DOCUMENT RESUME

ED 101 383 CS 500 907

AUTHOR Anderson, James A.
TITLE More Thoughts on New Directions in Mass Communication Research.
PUB DATE Nov 74
NOTE 11p.; Paper presented at the Annual Meeting of the Western Speech Communication Assn. (Newport, California, November, 1974)

EDRS PRICE MF-$0.76 HC-$1.58 PLUS POSTAGE
DESCRIPTORS Communication (Thought Transfer); *Educational Philosophy; Higher Education; *Mass Media; Measurement Instruments; *Media Research; Research Design; Researchers; *Research Methodology; *Research Tools

ABSTRACT

A clearer understanding of the capacity of research and researchers in the mass communication field is needed. In the past, when statistical researchers have found they cannot meet criteria, they have devised a new set of test characteristics. For example, when validity cannot be measured, researchers measure reliability, despite the fact that reliability is interesting only because it is a necessary component of validity. Mass communication research needs to acquire some new directions, including the development of a library of case studies on human communication behavior and the development of more sophisticated descriptive measures than sex, age, education, and so on. Finally, researchers in mass communication need to declare a moratorium on the uses of semantic differential, Likert-type scales, adjective check lists, shocking devices, and other measurement instruments which so poorly reflect reality. (RB)

U.S. DEPARTMENT OF HEALTH
EDUCATION & WELFARE
NATIONAL INSTITUTE OF
EDUCATION
THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL NATIONAL INSTITUTE OF EDUCATION POSITION OR POLICY

ED101383

MORE THOUGHTS ON NEW DIRECTIONS

IN

MASS COMMUNICATIONS RESEARCH

by

James A. Anderson
Ohio University

James A. Anderson

The author wishes to acknowledge
the contribution of Timothy P. Meyer
of the University of Massachusetts

WSCA Convention
Newport Beach, California
November, 1974

CS 500 917

Last year, at this meeting, we began a discussion exploring some new directions in Mass Communication Research. Today, I'd like to continue that discussion by outlining some areas where we quantitative research practitioners have failed.

To begin, we need a clear understanding of the capacity of research and researchers in our field. Speaking as one whose major thrust is in research and as one who, by all common measures, publications, speaking engagements, grants, consultancies, has been more successful than most, we are the alchemists of the 20th Century. The analogy is exceedingly useful. The physical alchemists are pictured as trying to gain wealth by dealing with the characteristics (thomistic "accidents") of objects without any understanding of the substance. We in turn are trying to obtain wealth through behavioral control by dealing with the accidents of behavior with no understanding of its substance. In this regard, one of the chief alchemists is B. F. Skinner. Let me remind you that the alchemists in their day were not laughed at. They were honored men. We honor ours, too.

Why are we alchemists? Simply because our capacity to ask questions is well beyond the methodology we have to answer them. When one dispassionately examines the welter of "paper and pencil" measures which purport to measure everything from intelligence to attitudes to sexual behavior codes and considers their success, the conclusion forcefully appears that we are in a perhaps interesting but nonetheless unfruitful, backwater of man's scientific development.

When I was first married, I worked in one of the automotive factories

My job was to check a aperture in the engine with a go/no-go gauge. If the gauge was "no-go", the aperture was to be marked and engine pulled off the line. Apparently it was difficult to produce this aperture as most of the engines were marked. I had worked but a few hours when in rapid succession the production manager, the crew foreman, and the union foreman had observed my work and had adjusted the standards until my little finger was the gauge. Production increased dramatically. Research has followed the same pattern. The methematicians carefully crafted models by which certain procedures could be developed. Statisticians came along and applied those procedures to large scale agricultural and production sampling where the conditions of control were good and the potential number of variables small. There was some warping of the models, distortions here and there which were nettlesome but not substantial. Psychologists came along and began large scale intelligence testing. In quick order random sampling disappeared (I cannot remember a single communication study in which the requirements for a random sample were met); the assumptions of the numerical scales were ignored (Are your attitudes in equal intervals and are your intervals the same as mine?) and the mathematical models themselves slipped out of consciousness.

What we have done in statistical research is that we have found we cannot meet the assumptions necessary and have, therefore, established a new set of characteristic tests and arguments that we can meet. We can't measure validity so we measure its anticedent--reliability. If we can't meet the norm, we argue around it. Reliability is only interesting because it is a necessary component of validity. Unfortunately, one must know reality before one can measure validity. A measure can be perfectly consistent with nature and totally inconsistent with the statistical procedures used to

determine its reliability. In short, our benchmarks are false.

Beyond our problems with statistical procedures, we have shown a consistent disregard for disciplined thought in our approach to the problems we attempt to solve. A little ruminating in the area of attitude change might provide an excellent example.

Scientists talk about attitudes and attitude change, but what they measure is behavior. The necessity for inference regarding attitudes would be of little concern if the relationship between behavior and attitudes were isomorphic. Unfortunately, it is not. Behavior can have multiple motivations; i.e., the same behavior can be evoked by different motivational systems. Behavior can also be directed to multiple goals. The behavior may be precisely the same, but the consequences for the organism entirely different. Prediction of a <u>given</u> attitude from a <u>given</u> behavior is, indeed, risky, as is the reverse prediction.

Attitude change suffers from the same lack of direct comparison. A behavioral change does not necessarily signal an attitude change. Behavior may change because the attitude remains constant. A golfer on the first tee is motivated to drive the ball straight and long. Selecting his driver, he hits the shot, slicing it badly to the right, actually adding yardage to the hole. On the next tee, he selects a different club, perhaps a low iron which cannot give him the distance of his driver, but one by which he can control the direction of the ball. Has his motivation changed? No, he still wants to hit it straight and long; something he cannot do with his driver.

The relation between attitude change and behavioral change, then, is not a necessary one. In fact, attitude change may occur without behavioral change (behavior can have multiple motivations), and behavioral change can

occur without attitude change (more than one behavior may be associated with a motivational system). Of course, behavioral change can be associated with attitude change. The point of this discussion is that the two do not necessarily relate to one another.

The complexity of the development of motivational systems strongly suggests that most behavioral change does not involve attitude change, and that such attitude change that does occur concerns the selection of different responses within a common hierarchy to meet the unchanged reinforcement contingencies. Motivational systems are either indigenous to the individual or developed over extended maturation periods. Such constructs are not readily tampered with.

The concurrent operation of multiple motivational systems can readily account for most behavioral changes and need not involve attitude change at all. Stimuli received under different motivational conditions will receive different meanings. Stimuli which appear to be the same for the observer or even the same stimuli repeated over time may not be perceived as the same stimuli by the subject, due to changes in the relative dominance levels of the operating motivational set. The importance of this concept is that it demonstrates that an individual's responses to an attitude object can be consistent but dissimilar from time to time, given the operation of different motivational systems. For example, stimulus Y under research conditions may produce response X, but under "real life" conditions, stimulus Y evokes response Z because of the operation of different motivational sets. As every communication researcher knows, messages persuasive in the laboratory easily become unpredictable in the "real world".

In our thinking, attitudes are associated with motivational systems. Motivational systems include the primary drives, instincts, critical periods,

secondary drives, values, beliefs, and other such variables introduced in the literature. Attitude change, then, is any modification of a motivational system. In behavioral research, attitudes are inferred from the consistent presentation of behavior given the same stimulus set. We are concerned about attitudes because documentation of their existence allows us to be more predictive of an individual's behavior.

We can readily identify two processes of behavioral change of which only the latter can be identified as attitude change:

1) Shifts in the dominance alignment of the motivational systems operating in a given motivational set. These shifts may result internally from changes in deprivation states as the dominant system achieves satiation and a new system comes to the fore. These shifts may also be the result of changing internal and/or external environmental information, which is interpreted as raising the probability of satisfaction for some system competitive with the then prime mover. These processes result in no structural changes in the motivational systems or response hierarchies; only the observable behavior is changed. A toy car may go forward or backwards depending on the position of a hidden switch. The "observable behavior," although perfectly consistent with the internal structure of the machine, may appear totally unpredictable. Most changes in behavior are the result of dominance shifts.

2) Restructuring of the meaning unit (interpreted stimuli set)/behavioral unit association bond. The mature individual has devised behavioral sets to deal with his motivational conditions. Further, the individual structures his environment (through societal, cultural, and individual processes) to provide a predictable set of stimuli. Given a set of stimuli interpreted according to the motivational set, the individual has available behavioral solutions. When the stimulus-response-reinforcement chain is well

made, the passage from stimulus to response becomes very efficient--habitual, we say. The strength of the habit is a function of its efficiency. The more efficient the response, the stronger the bond, the less likely change is to occur. Inefficient associations, however, are amenable to change. Inefficient stimuli are those whose appearance is reliably preceded by other stimuli and that appearance is not a necessary pre-requisite for reinforcement. Inefficient responses generally result from the following conditions: a) organism has had little experience with the particular requirements (usually external), and simply used the closest approximation existing in his repertoire; b) requirements are highly variable, inadequately displayed, and/or contradictory; c) operating motivational systems are competitive; d) dominant motivational system demands a specific response difficult to produce; and e) old response is no longer available.

Examples of these conditions readily come to mind. a) The new driver of a foreign car attempts to use the familiar, manual, gear-shifting pattern to find reverse--and fails. b) A teacher in attempting to motivate a new class of students is faced with a bewildering array of requirements. In fact, most social conditions contain these difficulties. c) A worker changes shift time and is forced to work when sleepy and sleep when wakeful. d) Responses difficult to produce are those with a large number of sub-units, precise sequencing, requirements of peak output, and/or contingencies outside the direct control of the organism. In the last case is the whole family of responses which call for the assent of other organisms such as consensual sexual intercourse, business contracts, and publications in review journals (see also condition"b"). e) The county clerk for whom you have voted for the past four elections has retired.

The probability of change under any of these conditions is a direct function of the strength of the motivational system and, as noted, the inefficiency of the response. Fashion, social products (personal soap, perfume, after-shave lotions, etc.), and those products whose results are to be judged by others (e.g., laundry products, household cleaning products, suburban lawn tractors, and for many, automobiles) are all responses highly susceptible to change when they are used to produce adequate reinforcement for the major motivational systems of sexual activity and social approval. The supposed inundation of sexual offers promised to those who wear some after-shave lotion or perfume rarely occurs. Yet, if that is the basis (however scaled down) for the response, then reinforcement can only be incomplete. As long as the individual perceives the perfumed lotion as a key to sexual success, and as long as the promise of the product exceeds its performance, rapid change among specific products is likely.

Voting per se seems to be a motivated behavior, while voting for a candidate seems to solve the motivation to vote. This analysis seems particularly useful in dealing with the partisan voter and the so-called independent. The hard-line party voter has no decision to make in partisan balloting other than whether to vote at all. Issues and candidates are essentially irrelevant, as party label is the deciding characteristic. He is motivated to vote a party. His motivational system is more efficient than the (true) independent's is. Once the decision is made to vote, the independent is then forced to determine for whom to vote. Candidates can be "merchandized" with the independent, but not with the partisan voter.

Changes in responses loosely associated with the meaning unit do not greatly affect the motivational system. Their primary effect centers on the

solution to the motivational problem presented. Consequently, they make little change in the "psychological status quo", of the individual, and can be accomplished with relative ease.

Whatever the value of this level of thinking, it ought to preclude the proclaimation of attitude change as the result of pre and post measurements interspersed with some message of the little interest to the respondents. The changes found in such studies are quite obviously not attitude changes but rather behavioral adjustments to a changed environment.

Last year when I spoke before this learned association, I concluded with some recommendations for new directions in research. I'd like to repeat three of those at this point and to add two new ones.

We need to develop more refined measures of individuals. Simple descriptive measures such as sex, age, education level, and the like are such broad categories that it is not surprising they have shown little utility in the many studies that have used them. Obviously, it is not age the affects media selection, but the concomitant attitudes, needs, and emotive states which have developed over time.

We need to systematically distinguish among reception behaviors. To be drifting in and out of some media fare while finishing furniture cannot be treated the same as 4th down, two yards to go, 30 seconds to play. The condition of and the effect on the individual must be vastly different. In a similar vein, we need to more thoroughly describe the conditions of reception. Differing environments have differing effects on the result of communication behaviors.

We need to direct our attention away from content. Content per se has little value; it assumes value only in relation to the response the

individual generates. Content variables have been notably weak in predicting that response. The chaos of the violent-media-fare-leads-to-aggression results is a clear indicator of that failure.

As to my new pleas, we need to develop a library of case studies relating to human communication behavior. Nowhere in our literature can the research scientist turn to find adequate descriptions of the ordinary behaviors that do occur when individuals watch television or read a newspaper. A catalogue of descriptive narratives would be an invaluable aid in tempering the effects of our own biases.

Finally, we need to declare a moritorium on the uses of the semantic differential, Likert-type scales, adjective check lists, shocking devices and other such measurement instruments which so poorly reflect reality to us. And during that moritorioum, we need to expend our creative energies on solving the methodological problems in the direct yet inobtrusive measurement of human behavior.